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CONSIDERATIONS IN CONDUCTING BIOTECHNOLOGY IMPACT STUDIES

Walter L. Fishel
The Ohio State University

INTRODUCTION

There has been a great deal written in the past couple years about the impending technological revolution in agriculture from biotechnology innovations (NRC,1982;NAS,1984). The tone of these writings suggest impacts on agriculture that have the potential for being substantially greater than those from any previous technology in agriculture. Further, considering the nature of many of these developments, there may be few economic and social structures that will remain unaltered to some degree because of the source, nature and rate of change in biotechnologies. Biotechnology impacts in agriculture do appear to be a prime area of examination for those of us interested in assessing technological change.

There is a rich history in studying technology change and its impact on the agricultural industry and related institutions and structures (Hayami and Ruttan, 1971). Based on these studies, we do have a reasonably solid foundation of techniques for examining most technological change situations, at least as conventionally perceived. At the same time, there are many things about the evolving biotechnology industry that causes me some concern about the adequacy of these analytical techniques to allow us to generate the information that will will be needed in the coming years.

This paper records some of these thoughts and associated concerns. In doing so, the paper is aimed more at the need to reexamine our philosophical conceptualization about how we even consider technology assessment studies of biotechnology than it is at analytical techniques themselves. My judgement is that once we have a firm anchor for our perception of biotechnology assessments, the appropriate techniques will come. This approach to the topic is diagnostic rather than curative. And, it is cast within the context of microeconomic considerations rather than macroeconomics; to some extent my comments are less relevant to the latter. Finally, this discussion should not be taken as any kind of moral judgement about past technology studies or the analytical methods used in them; it is merely an exercise in raising questions about issues involved in the match between topic and method.

I will briefly discuss examples of analytical techniques but will not provide a survey of methodologies. A survey as such is a less important topic at this point and, certainly, others are better equipped than I to do the job. In discussing methods and techniques, let me

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refer for convenience to the kinds of methodologies normally encountered in agricultural economics and sociology literature as the Traditional Methods (TM's), excellently represented by the Sundquist, et. al. (1982) publication on technology assessment of corn production, and those not usually encountered in TM's as Unconventional Future Oriented Methods (UFO's), a not altogether inappropriate acronym.

The paper first considers the issue of the need for a reexamination of our philosophical orientation in conducting biotechnology impact assessment studies, what is involved and why it is important to make such an examination. Next, some characteristics of biotechnology change are presented that suggests the possible need for reexamining our study design and analytical approaches. I then examine some of the possible effects on our study design and analytical methodologies that can be expected to result from accepting a different way of looking at biotechnology impact assessment studies, followed by a few general conclusions.

THE NEED FOR A PHILOSOPHICAL REORIENTATION

The role of one's philosophy, reflecting how we perceive the portion of the world that contains those problems of interest us, is obviously important regardless of the study topic involved. Careful consideration of one's philosophy may be the most important consideration when it comes to conducting studies of biotechnology impacts, particularly for social scientists. In particular, there would appear to be three main areas of one's philosophy that need special attention, namely

1. The conceptual or theoretical framework and set of analytical tools which are used to consider and solve problems.

2. The real purpose (versus justification) put forth for conducting technology assessment studies.

3. The individual analyst's perception of the nature of the environment being studied, in this case the path of future biotechnology developments and the nature of resulting impacts.

The question that arises from consideration of these three areas would be something like, "Is there something about the subject of biotechnology impacts that alters (or should alter) our perception of these three considerations as applied to past technology assessment studies?" If so, how should these changes be reflected in

1. study identification and specification,
2. study design,
3. problem analysis, and
4. data collection.

At times, our views about what policy/decision issues we intend to address in our studies, the conceptual frameworks we have used successfully to date to consider and analyze problems, the analytical techniques that are the accepted standards in our profession, and commonly accepted data sources tend to become meshed into an undiscernible entity where the individual effects of each of these become hidden in the overall study design. The end result of all this is that determining what is really going to "drive" our studies becomes a process of accommodation. And, like most accommodations, the value of the end product is likely to be much less than we intended but probably more than we deserved! To the extent that we can successfully sort out the individual effects will be the extent to which we can improve both the quality and usefulness of the information produced by our studies.

Conceptual Framework and Analytical Methods

The term conceptual framework simply reflects the fact that each of us by discipline, training and experience has developed over time a logical informational and analytical structure which we almost automatically apply when considering problems for analysis and solution. Each of our disciplines orders and relates knowledge in a certain way, guiding us in specifying what parts of a problem being considered is relevant to our realm of analysis. Based on the same training and analysis, we also tend to accept a particular set of analytical procedures and data acquisition methods as being useful to this problem solving, naturally tending to stick with those with which we are familiar and especially with those that have worked for us in the past.

Both of these play a large role in both determining what problems we will choose to study and how we specify the problem for analysis. And, while we recognize this posture to be limiting, the alternatives are generally considered inefficient or at best impractical. As an example, economists utilize a logical information structure based on value and choice usually in a framework of comparative statics, while systems analysts deal with performers and functions and are concerned with processes. Each may use the other's concepts, but only as tools of analysis, not as the logical vehicle for problem conceptualization. In any case, whatever the conceptual framework or discipline-oriented theoretical structure involved, we recognize that some subset of the actual world is being forced into some sort of artificial configuration for examination.

The extent to which this is an important consideration depends on what information and/or new knowledge we want to gain from the examination. But, at the outset, we need to recognize that we are taking a problem that has a logical structure of its own, if we were indeed smart enough to determine what it is, and redefining a part of it into a totally artificial configuration that is meaningful to us, one in which we can deal with the problem. But, in doing so, what important characteristics of the problem are left out? To what extent does this limit the validity and range of usefulness of study results? To what extent are we accommodating product for the sake of logical structure and method?

Study Purpose

I consider the last question particularly important. Especially for biotechnology impact studies, it seems to me to be a critical task to clearly identify what the "driving force" in our study design is going to be in order to provide unequivocal guidelines to the further development of a study. It is assumed in the following discussions that tenure and promotion considerations are at least a secondary influence as <u>the</u> driving force, although realism precludes taking this assumption too seriously. Certainly, the impact of this latter influence on what we select to do cannot be underestimated (Schuh, 1984).

There are many <u>reasons</u> why we conduct research, including such things as making a living and intellectual curiosity. I would submit that there are really only two <u>purposes</u> for studies that we could denote as being primary, namely

A. To supply policy and decision makers with information they need to make choices about future events.

B. To marginally increase our discipline-oriented knowledge about technology impact phenomena.

The differences in the general impact of these purposes may be characterized as follows:

A. Information required to satisfy the policy/decision act is the driving force of the study. The model design becomes paramount and is completely specified first. The analytical technique and data specification and acquisition characteristics are adjusted as needed to provide the required information, however good or poorly these adhere to criteria of precision, etc. The primary role of the information is to help anticipate the results of future events. The most important standard is the degree of reliability users attribute to the information.

B. The entire study is precision driven, i.e., the model design and analytical techniques are adjusted to reflect data availability and accuracy. Policy/decision questions that may be answered by the information generated are accordingly adjusted. The primary role of the research is to understand more about a certain phenomenon. The most important standard is peer acceptance.

The one is information driven while the other is driven by study structure and data. Obviously, these are not the same thing, nor can both primary purposes seldom be achieved equally in a single study. Practically all research we do falls under purpose **B** with the researcher assuming that the results also will be relevant for purpose **A**. But, this is true only to a limited extent. The reason for this is that in practice one can differentiate three levels of information going into a policy/decision act: expert knowledge, intermediate information and decision information (Coates, 1977). Nearly all of our studies fall under the category of expert knowledge. In general, this constitutes in-depth information about a relatively confined set of phenomenon. The more one restricts the subset of the real world to be studied or the more restricted the disciplinary involvement, the more confined the expert knowledge is. It is up to individuals or groups operating in a staff function role to combine sets of expert knowledge with other information into an informational framework relevant to the eventual policy/decision maker, or intermediate information. In the actual decision act, other information is added to this latter set, frequently in a "black box" mode,to produce the eventual policy or decision.

This simple consideration, obvious to all, has some significant ramifications that are not easy to deal with in practice. Peer review may be the biggest problem of all. The above suggests that we may need to consider a completely different way of conceptualizing and conducting our research if we were to concentrate on option A. Even if we take the time and trouble to learn new analytical and data acquisition methodologies, would our peers, who do not care to make a comparable effort, accept the results? For example, can we wean ourselves away from the overwhelming preoccupation with precision in our analysis? Can we adjust ourselves to a "looser" form of analysis that is designed to help anticipate change rather than understand it? Again, these are not simple decisions to make, especially when there is so much we can do using TM's that will better help us understand the basic impact phenomenon.

Study Environment

The analytical models of problems to be studied are dependent on what we observe to be the nature of the world, or that subportion of interest, that we intend to study. While this holds true for all studies, it has a special significance for biotechnology impact studies in one particular respect. That is, what do we as individual analysts believe will be the nature of biotechnology changes and consequently how will these impact on the agricultural industry? On the one hand, we may believe that the adoption of biotechnology innovations will be gradual and possibly sporadic, not differing significantly from the pattern of the sum of past technology adoptions in agriculture. Impacts of these adoptions will in all likelihood be gradual over the long term. Such a view would be consistent with views of the world that does not differ significantly from those of researchers who have been conducting technology assessment studies over the past several decades. On the other hand, we may believe that biotechnology introductions will not be gradual but at times almost explosive, providing impacts on agriculture that is neither gradual nor continuous. Such a view may cause some concern with the applicability of TM's to the study of biotechnology impacts and lead us into examining the appropriateness and effectiveness of our analytical models and techniques.

CHARACTERISTICS OF BIOTECHNOLOGY CHANGES

Earlier, I asked the key question, "Is there something about the subject of biotechnology impacts that alters (or should alter) our perception of these three considerations as applied to past technology assessment studies?" I would now like to address this question, because I think there is. The discussion is limited to only those factors that I consider the more significant ones to this discussion and is abstracted from Fishel and Kinney (1985).

The principal source of change in the study environment originates from the extent to which biotechnologies are being developed and disseminated outside the traditional sources of agricultural technologies. While many technologies have come from other than public sector research institutions, especially in recent years, nothing approaching such a stream of private sector technologies has ever been encountered before in agriculture. The significant effect of this will be reflected in a radical change in the source and control of technology related information, in its relative dependence on the information source and consequently its relative objectivity and availability.

In addition, the nature of the technical changes will be much different than most of the past technological changes that have been studied. Biotechnology innovations and introductions are being driven by large infusions of capital from outside the traditional sources of new technolgies, namely from the private sector. These efforts are being applied only to areas of high technology in which the high risk of development is associated with high expectations of profits, and all that goes along with it. The resulting biotechnologies will have incremental and disjointed impacts on agricultural productivity. Only the hybridization of corn and possibly the current adoption of computers and microprocessor control systems can be expected to have a comparable impact to what may become commonplace for many biotechnologies.

Nearly all past technical changes in agriculture have been gradual enough for adjustments to be partially governed by tolerable rates of capital consumption. The decline of marginal operations have been at a rate that acceptable, if not preferable, adjustments could be made. The nature of the high technology, capital and knowledge intensive biotechnology based systems will greatly aggravate the adjustment problems by accentuating the differences in levels of productivity between capital and knowledge intensive versus more labor intensive systems and units. Rates of capital consumption will be less important as a decision factor in adoption and, consequently, adoption rates will be greater to an as yet unknown degree than for most past technologies. The significant implication of this is that issues related both to management decisions and to governmental adjustment programs will need to be anticipatory rather than reactive.

Because relatively few biotechnologies have yet come on stream, possibly the most deceptive and least recognized causes for concern in this changing technological environment is the sheer magnitude of the number of new biotechnologies that are or will be developed. Changes will be coming from many directions simultaneously. Virtually every biological aspect of agriculture is the subject of biotechnology innovations. And these are occurring concurrently. At no time in the history of agriculture has such a proliferation of new technologies been introduced in such a short period of time with each one having potential for substantial impact on productivity. Such an occurrence presents few possibilities for segmenting out relatively narrow areas of agricultural technology for individual study. Further, much of the main thrusts in biotechnology remain in the basic research stage. Finally, an increasing share of new developments are in industrial laboratories hidden behind the cloak of proprietary information (Kenney, forthcoming).

EFFECTS ON STUDY DESIGN AND METHODS

The foregoing is not intended to suggest that existing models and methods for assessing technology impacts are to be replaced with a whole new order of models and methods, or UFO's. Existing methods will still be as useful and necessary in future studies as they have been in past. The contention here is that there must be some analytical extensions to these methodologies in order to generate the kind of information that is going to be required by decision and policy makers.

As a synoptic overview of the above in terms of its effects on studies of biotechnology, there would appear to be a few underlying considerations that seems to permeate everything else that we do. One of these is achieving a clear commitment to understanding why we are doing the study in the first place. In particular, how strongly is the study design and analysis going to be information driven. A corollary to this is how necessary is the concept of precision in the study methodology? That is, how willing are we merely to anticipate change and impact relative to our need to understand it? Another underlying consideration is whether or not we accept the key role of uncertainty as an element of information equal in importance to the anticipated impacts themselves. Considering the nature of the subject we are to study, should we not be as interested in the predictability of events as we are in their anticipated effects?

In brief, the significant factors affecting study design and methods from the preceding are as follows:

1. The paramount feature is the future orientation of biotechnologies, with relatively few actually existing at the present time. Obviously, this has strong implications to the task of identifying and specifying the particular biotechnology we intend to study.

2. Data needed for analysis does not yet exist, it is not available because of proprietary reasons or that which does exist and may be available is too complex for cost-efficient acquisition.

3. There is a high probability of disjointedness of effects because of the type of biotechnologies introduced and expected rapid rates of adoption.

4. The scope of biotechnologies introduced in agriculture will be pervasive, suggesting some difficulty in effectively handling the logic of externalities in our study designs.

Accepting these influences to whatever degree is palatable, how then do these factors impact on how we conceive and put together studies on biotechnology impacts? First of all, there is no little difficulty in deciding how to logically order or even contain such a discussion. All of these factors can affect in almost an infinite number of ways how we design and conduct our studies, especially considering the range of possible topics on which we can conduct biotechnology impact studies. I will expediently resort to discussing only those topics that have concerned me the most. The rest are left for posterity to deal with.

Future Technologies

While there are a number of biotechnologies that have already become commercial realities, or are very close to it, most of the ones expected to have the greatest impact on agriculture have not. Although we could take existing ones for study of impacts, I would maintain that many of the problems indicated in the preceding would still exist. In addition, only a very few of these are of any real significance.

In any case, in order to anticipate the impacts of a biotechnology, we first have to describe the biotechnology that will cause impacts. And, especially in the case of biotechnologies, some of these can be very complex in terms of associated technologies and delivery systems required to make them operable. Actually, predicting new technologies is not a new or entirely unique kind of activity. Discounting futurists' dreams of things to come, there have been many studies aimed at trying to foresee what technologies in the future will be and/or what effects they will have, generally refered to as technological forecasting (Martino, 1972). These provide reasonably good guidelines for describing future technologies, even though most are concerned with engineering topics. I am not familiar with comparable studies in agriculture.

In describing these future biotechnologies, we are doing essentially the same thing as we do in describing an existing technology before analyzing the impacts it has had. Except in the one case we are looking back and in the other we are looking ahead. The nature of the one we describe from existing facts, the nature of the other we must conjecture. However, in describing a future technology, there is one additional key element that must be determined: when it will become commercial. Only then can we start considering things like rates of adoption, leading to the assessment of the eventual impacts.

There are several elements of information that must be developed in order to describe a future biotechnology, namely

what the expected biotechnology will look like and what effects on production, marketing, or whatever, are expected;

the anticipated scale of effect of the new biotechnology if successful;

when can we anticipate it will become commercial;

what other technologies must be in place for it to be viable;

the delivery systems required for successful implementation;

the path(s) and sequence of new knowledge that will need to transpire to achieve the commercialized biotechnology;

the probability of achieving the new knowledge required;

how certain or predictable is this set of events; and,

possibly, what are the resource costs involved in achieving the new biotechnology.

While this is an onerous set of information to be determined, a logical, stepwise approach to its development at least results in a task that is not overwhelming. Nevertheless, it will add an additional, significant component to any study of biotechnology impacts.

How to develop descriptions of future biotechnologies is a topic for separate treatment. However, simply as an example of such a description, I will only briefly describe how one such description is being developed in a project on studying the socio-economic impacts of the growth gene transfer in swine. This biotechnology results from transfering a gene that controls growth rate from the rabbit DNA into the swine DNA, proven feasible at Ohio University by a successful transfer to mice with perpetuation of the characteristic to subsequent generations.

The framework for analysis utilizes the concept of <u>decision trees</u> from operations research. In this case, I refer to the structure as a <u>development map</u>. The originating node is a relatively comprehensive description of the characteristics of the final commercialized biotechnology, including associated technologies and delivery systems required to bring the technology into actual use. The first tier of branches lead to nodes that indicate the new knowledge that will be required to achieve each of these characteristics. The next tier of branches lead to nodes that indicate knowledge that must precede this new knowledge. The mapping continues until a node on each branch identifies knowledge that currently exists. Not all of these characteristics or knowledge requirements are necessarily biological or physical ones; some may be economic or sociological, as in the case of the need for new organizational configurations or specialized educational requirements of potential users. Although the initial development map is prepared before contacting "experts", the experts may recommend alterations based on their particular perspective. Panels of experts, identified by their degree of familiarity with the state-of-the-arts for each specific node, are then surveyed to provide subjective estimates about information like that listed on the preceding page. More about the survey and estimation methods is discussed in the next section.

One key point that should be obvious from the forgoing is that this activity requires working very closely with bioscientists in the capacity of co-researchers, not simply relying on them as data sources. Economists and sociologists cannot do this alone. There is simply too much technical knowledge required and too much time required of scientist(s) up front to expect them to participate only in an ad hoc fashion. From start to finish, this is truly a multidisciplinary activity.

An interesting question that will be raised in this area is how far off in the future can the biotechnology be that we select for study? I would submit that we could develop a description of any biotechnology that is currently being discussed or, for that matter, imagined. However, as a practical matter, we should only consider those for which some of the "foundation" discoveries are beyond the basic research stage, i.e., feasibility has been proven. The practicality of this limitation arises from the need to gain an acceptable level of response from the scientist experts whom we ask to provide information. It is difficult enough to obtain their cooperation. The farther one goes beyond the current boundaries of the state-of-the-arts, the more difficult obtaining this cooperation becomes.

Data Sources and Acquisition

Data is going to be an even bigger problem in conducting biotechnology impact studies than it is and has been in other types of studies. Aside from the usual problem of finding data series on past events that we need and can afford to collect, proprietary restrictions are going to be a big hurdle to overcome. Also, many of the events in which we are interested have not happened yet, so neither have the measures of those events. Consequently, I am convinced that we, out of necessity, are going to have to rely more and more on "expert" opinion, on those who have the best knowledge about some aspect of our event. Obviously, what I am talking about is a greater role in the use of subjective estimates. It is this aspect of data sources and acquisition that I want to stress in this section.

Suggesting a greater role for subjective estimation in our studies does not suggest that it is better data in any way than that obtained from primary or secondary sources. The principal point to be made is that use of subjective estimation techniques provides an effective alternative to placing unnecessarily restrictions on the information product of a study because of a lack of secondary data and/or prohibitive cost of generating primary data, if one assumes the driving force of the study is to generate information to benefit policy/decision makers.

It is interesting to me that despite the fact that all of us have used subjective estimates at some point, data obtained by subjective estimation is still looked on as a second class method of data collection. Attitudes toward subjective estimation techniques are so poor that we do not even consider providing any training for our students on this useful topic. Part of this may originate from the fact that we do not consider "data chasing" a very popular task. We would rather let a GRA do it. This is regrettable, because it prevents our recognizing that, beyond being a necessary thing to do, subjective estimation techniques can be in their own right a relatively powerful tool.

Such an attitude exists despite the fact that subjective estimation is not at all a new phenomenon. Weather analysts were developing relatively sophisticated subjective estimation procedures in the 1920's. Starting in the late 1950's, techniques such as Critical Path Scheduling (CPS) and the Program Evaluation and Review Technique (PERT) made subjective estimation legitimate. In the late 1960's and early 1970's, DELPHI opened up a whole new domain of investigation utilizing subjective estimation. Now, in the 1980's, "expert systems" and "artificial intelligence" are starting to bring scientific dignity to the subject.

Generalizing, "subjective estimates" is simply a response by a person reflecting beliefs about some event in terms of some scale. The scale can have almost any units, although they are usually in terms of binary choice, cardinal units, or ordinal units. However, we could also have subjective estimation involved in identifying and specifying the scale itself, as in the early stages of creating a DELPHI scenario. Likewise, the subjective estimates may be in any one of a number of forms, including (a) ones that are purely narrative, as in describing a DELPHI scenario, (b) some sort of point estimate, with which we are most familiar, (c) simple rectangular or triangular distributional estimates, commonly used in CPS and PERT, or (d) subjective probability estimates, which utilize more sophisticated probability distributions.

One important distinction should be made to improve discussions about subjective estimation techniques: although the general class of methods known as DELPHI utilizes subjective estimates, all subjective estimation are **not** DELPHI. There are many ways subjective estimation can be used outside the DELPHI configuration, in particular (a) any place we use secondary data (if desirable to do so), (b) any situation in which data is not available for whatever reason, (c) where it is a noticeably less expensive way of obtaining data than other primary or secondary sources, or (d) in situations where an event is not easily specified.

Again, I will only provide a brief example of the use of subjective estimation, using the growth gene transfer study and the attempt to describe a future biotechnology. Using the development map described on page 9 as the framework, there are a number of elements of information that must be obtained. For example, I need to know the length of time expected to elapse between each of the nodes, the nodes being the sequential new knowledge required to achieve an eventual commercialized biotechnology. However, even this single variable represents a very complex event to quantify. The new knowledge at each of the nodes is not well defined, in some cases almost conjectural. Each expert visualizes future possibilities differently depending on his disciplinary background and experience. Some experts simply have a more optimistic or pessimistic nature than others. In any case, there are two important measurements in addition to the estimate of time itself that must be generated. One is the amount of uncertainty associated with the time estimates. The second is some idea about how predictable are the events.

The technique used is essentially one I developed a number of years ago at Minnesota to obtain information for calculating ex ante cost/benefit estimates for selected research projects (Fishel, 1971). I called the method "impression measurement". It's function basically is to get the impression of experts about an event in a form that can be used in a computer. Although the calculations are somewhat involved, the procedure is relatively simple. Given a statement that describes the new knowledge at any two adjacent nodes of the development map, the expert is asked to estimate the time he expects will be required to achieve the second set of new knowledge assuming the first set is known. Five estimates are requested, the two 100 percent certainty limits, the 67 percent certainty limits and the most likely estimate (in that order). Based solely on these five estimates, a unique Beta distribution can be generated representing experts impressions about the time required. The Beta distribution was selected because it can assume any shape from rectangular to cubic to normal or anything in between and with any degree of skewness and kurtosis based solely on these five estimates. It is also very handy for computer applications because its shape is entirely dependent on its two parameters and has the standard range of 0 to 1. A weighting scheme is employed to combine estimates where a panel is used. The resulting probability distribution is then used as the measure of the time variable in any subsequent analysis. Employing Monte Carlo techniques, the uncertainty is preserved throughout the analysis. The distributions also provide the basis for generating an index describing the relative predictability of the various events estimated.

One issue that frequently arises in considering subjective estimation is how one validates subjective estimates. Oddly, this is both a relevant and an irrelevant issue! First, we must distinguish between two types of data:

A. estimates about existing fact or events, and

B. estimates about "immeasurable" current or future events.

Only **A** can be checked for validity in the usual sense, i.e., estimate, then measure, then validate. Except for designed experimentation on validation, **B** is not subject to validation in the usual statistical sense. Because of our analytical penchant for precision, we usually are thinking about validity in the sense of **A** and, consequently, have some difficulty in conceptualizing the role of type **B** information. Subjective estimates are most powerful in situations in which it is not otherwise possible to measure an event, for whatever reason. Consequently, there is in effect no possibility to take measures against which one can test validity. If there were, then the measures should have been used in first place. Hence, the role of **B** is to provide an effective alternative to simply ignoring the event altogether in the analysis. The only possibility for obtaining measures of **B** is to wait for some time in the future when the event for which the estimates were obtained runs its course. But, even then, considering validity for **B** according to criteria relevant to **A** is really only testing how good a predictor the estimator is, and by that time the world would be different than the scenario on which estimates were obtained, making the validity check invalid. When discussing **B**, it is more common to refer to "performance" than to validity. I recommend a section in Linstone and Turoff (1975, pp. 227-319) for a discussion of this rather involved topic.

Model Design and Analytical Methods

These two topics are considered together because in practice the two are usually, but not always, integrally tied together. Further, I do not propose any radical change in either models or methods. Even if the models that we have been using were inappropriate for future studies of biotechnology impacts, they are still going to be used. The practicality of the matter is that how we conduct our research as a profession simply does not change that rapidly. What needs to be considered is how models and methods may be changed marginally to reflect the concerns expressed earlier.

As a starting point, I find no ready evidence that would alter the belief that it is the nature of events being studied that determine the type of model that is appropriate, not the time dimension of the subject matter. Hence, there is no reason to believe that because something occurs in the future, apriori, the model structure will be any different than that which is appropriate to studies of past or current events. If econometric models, mathematical programming models, input/output models, system analysis models, etc., represent appropriate depictions of current technological relationships, they also should be appropriate for the study of future biotechnology impacts. However, one should expect significant diversions from this perspective in specifying the actual model design.

One of these, mentioned earlier, concerns how one perceives the nature of the future path of development of biotechnology innovations and adoption. For example, by utilizing historically derived coefficients in LP and I/O models, in particular, anticipations about the future are automatically linked to the past. Hence, even relatively simple LP and I/O studies basically can only answer the question of how we are going to trend away from the current status, not what the future will be like. While this is methodologically more comfortable, and certainly more expedient, the value of resulting information would be much improved by considering types of models that permit internal shifts in factor coefficients over time. In addition, or alternately, the model design may need to incorporate separate procedures in the overall investigation to identify the possibility for and nature of such changes. This point also would seem to apply in the following considerations.

If one accepts the perception of the path of development and adoption of future biotechnologies described in the section "Characteristics of Biotechnology Changes", the pervasiveness of biotechnology introductions and their inherent complexity will create greater model design problems in our attempts to deal with disjointedness and externalities. I do not have a very strong perception of what a generalized model that can handle these types of problems would look like. Until some substantive attempts are made, one can only conjecture.

For example, the problem of disjointedness, resulting primarily from very high adoption rates, suggests to me that a different kind of approach to developing models may be required in order to generate beneficial information for policy/decision makers. In current technology assessment studies in agriculture, we tend to treat dairy producers, for example, as a single observable unit. That is, all dairy producers are included in our models with significant variations in their characteristics handled through inclusion of variables. This has the recognized effect of averaging out measures of the effects we wish to identify. I would conjecture that in order to adequately handle the degree of disjointedness that we may expect from biotechnology adoption, it may be necessary to segment the population into separate study units, possibly based on these same variables, and then treat the units as separate populations in the analysis. Further, the methodology should permit members of one population to shift over time from one study unit to another according to changes in their characteristics. Some study units might be left out completely in the analysis, simply as not being relevant. The reasoning for the segmenting is that the separate units may become as unlike each other in terms of adoption rates and resulting impacts as, say, all dairy farmers are different from factory workers. I am not aware of any current model or method of analysis in which such an approach has been applied. However, this is the kind of innovative modeling and methodology that is needed.

Effectively handling externalities in our models may be the most difficult problem of all. Traditionally, we tend to decompose an environment for study down to a size that only a minimal number of externalities, treated as "givens", are explicitly recognized in our model. The minimal number is achieved by grouping external forces in units according to the nature of their effect. But, what happens when the externalities are so numerous, diverse in effect and pervasive that they become as important as the variables included in the model? Again, traditionally, we tend to handle this by expanding the scope of the model to internalize at least some of the externalized factors. There may not be much alternative to this. Model scope simply may have to be expanded sufficiently to minimize the effect of at least the principal sources of externalities. However, two options might be useful. One is to internalize some of the externalities, ones that appear to be extraneous to the main thread of study considered in the traditional sense, but include them as separate components from the main body of analysis. A second is initially to simulate the entire domain that includes the bulk of the externalities, even though eventually only one sector of the simulation is to be studied in detail. In both cases, additional analysis will be required to approximate the nature and general level of interaction between the segregated components or modules and the portion that makes up the main thrust of study. An additional requirement using such an approach would be the need to allow for recursiveness in the analysis. Either approach would allow us to retain relative simplicity provided by conventional models and analysis yet provide at least some of the benefits of more complex models.

In a slightly different vein, accepting the premise that providing information to benefit policy/decision makers is going to be the primary driving force of biotechnology impact studies places substantial requirements on how we go about designing both the model and the analytical procedures. In a very real sense, the model specification must be considered the independent variable and the data requirements and analysis technique the dependent variables. That is, information required is determined first and locked in place. Only then are the appropriate model design, data requirements and analytical technique determined. This is contrary to conventional wisdom which basically accepts the premise that we have a set of analytical tools in search of problems for which these tools are appropriate. Now we must search for the appropriate method. In addition, to the extent that subjective estimation procedures are to be used, information management techniques must be built into the model and analysis. This a proactive orientation rather than a reactive (or interactive) one as in traditional methods of model building and analysis.

My own biases tend to lead me to believe that the most useful models will be the larger scale simulation type more common in macroeconomic studies. These will be in modular format based on functional attributes that will lend themselves to being segmented for separate investigation, for example by graduate students. Analysis will provide for recursiveness among the modules. All of the currently used analytical techniques will be applied not as the method but as components of the overall analysis. And, both secondary and subjective data will be used separately and interconnectedly, i.e., in Baysian learning functions.

CONCLUSIONS

Members of our profession will find much to disagree with in the foregoing. After all, much of what I have proposed goes against the grain of standard thought and method. It would be interesting to be able to determine how much of this disagreement is based on real differences in a dialectic sense and how much is a product of the defensive mechanisms of the brain to radical diversion from habitual modes of thought. Much of this is not an easy transition to make. In fact, I too have problems. In dealing with the detail associated with precision oriented studies, there is a natural inclination to become so impressed with the artistry of technique that the philosophical whole is ignored or at least missed. It is much like the old adage: in keeping your nose to the grindstone, the grindstone is about all you can see! However, even having considered the philosophy of the whole, one still tends to react with wonderment when considering the applied techniques. One begins to question whether it is wrong to question these methods. After all, they are marvelously rigorous, and anything that rigorous surely can't be wrong. Again, I refer you to Armstrong (1978) for an excellent treatment of this problem.

It is difficult to step back from a path of action that has been so successful and ask, "Is there something else I really ought to be doing?" or "Is there a better way?" The American experience has been that change of more than a marginal character is not induced until a situation reaches a critical stage. Note the current situation with farm programs. The same is true of our profession...or nearly any profession, for that matter. Peer review and acceptance is the "driving force" of any profession. This is a consensus type of acceptance of what we do. While a necessary condition for any profession, in order to maintain even a modicum of stability, it can also be debilitating. Response to significant changes in the environment are lethargic when the responses required are contrary to consensus norms of acceptable professional philosophy and method. Thus it is with the coming of biotechnology impacts affecting agriculture and its infrastructures.

The challenges to social scientists involved in biotechnology impact assessment studies are substantial ones. The nature of the environment within which biotechnologies are being developed is different than is commonly realized and certainly different than that of agricultural technologies experienced in the past. The nature of the resulting impacts will be more pervasive, more complex and occur at a more rapid rate than anything previously experienced. The practice of making management and policy adjustments <u>after</u> an impact has occurred and its effects become clearly visible in the market place or to the general public will result in very great economic and social losses. The role of and challenge to the social scientists, as it has been in the past, is in providing the information that will help anticipate these changes and impacts and their economic and social costs. The circumstance that is different now is that the task of providing truly useful information will require a significant reexamination on the part of social scientists of how they go about conducting their studies.

Undoubtedly, the greatest challenge will come in modifying the way in which we conceptualize the overall problem and formulate our analytical structures. The information required by decision and policy makers must become the preeminent force in study design. Expected disjointedness in impacts suggests the need to reexamine traditional techniques of data acquisition and analysis. More than ever, we will need to consider including the methodologies used in other disciplines. At the same time, we must develop theories that better anticipate the general trajectories of technological, economic and social change that evolve out of the peculiar circumstances created by biotechnologies. Finally, there is a great deal of urgency in getting social scientists started on examining these issues. There are a number of methodological problems in particular to be resolved before social scientists can expect to provide a steady flow of useful information to managers and policymakers.

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